Genetics Department
University of Wisconsin
Madison, Wisconsin
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Professor Robert Tulasne
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My dear Professor Tulasne:

Thank you for your letter of December 1 just received. I have, of course, been greatly interested in the progress of your work on the L-forms and will be grateful to receive the reprints to which you refer. If you have not already done so, I would particularly appreciate receiving a copy of the review which you wrote in the "Revue D'Immunologie" two or three years ago but, of course, if your supply is limited please ignore my request.

I am enthusiastically interested in your accomplishment of converting various strains of Escherichia coli into L-forms and will be happy indeed to collaborate with you on this subject in any way that I can. May I suggest the following two approaches which are, I believe, consistent with your own suggestions.

First, I am happy to send you under separate cover a set of mutant strains of the K12 line of E. coli which is, of course, the material that has occupied the bulk of our attention for the past several years. If you can succeed in obtaining permanent L-forms from these strains for the purpose of further investigation, so much the better, and I will look forward to hearing from you on your success in this enterprise. If, however, this proves to be unduly difficult, it might then be equally appropriate for you to send us those strains of Escherichia coli which are most amenable to your experimental technique together with whatever details might be necessary for us to reproduce your own experiences. It is difficult to believe that this morphological cycle has no genetic interest.

I must record, however, that the remarks that I made in 1951, to which you referred in your letter, have since been superceded by more recent work. Although there is no doubt that bodies resembling L-forms have been present in many of our Salmonella cultures, we have not been able to connect these with either the process of transduction in Salmonella or with the sexual recombination process in E. coli. In the former case the active agent now turns out to be simply bacteriophage, whereas in the latter we have good evidence that the intact cells of the two parents are directly involved. This is not to say, however, that the continued investigation of the L-forms by genetic along with other techniques is unlikely to lead to results of the deepest interest, but what these results would be likely to be I am not in a position to predict. For the sake of clarity let me repeat again that the "FA" referred to in my 1951 paper has now been identified as bacteriophage rather than as L-forms but that I am entirely convinced that the L-forms must have some deep biological significance of which we are not at present sufficiently aware.

I have been particularly interested in your suggestion of a phylogenetic relationship between the L-forms of Proteus and the Rickettsia. Have you had any further experiences that would bear on this question?

Yours very sincerely and with best wishes of the season,

/mg

Joshua Lederberg,